CORRESPONDENCE

Sir,—I must thank Dr Condon most sincerely for pointing out the error in respect of the date of the R. S. M. Section meeting. It was, as he writes, held on the March 1 1946. The error was entirely a result of my lapsus menti and not "typographical". The other dates in the published summary of my contribution to the "Waterton" meeting were correct. I am so pleased, too, that he mentioned other workers of the time, especially Stanley Rowbotham—a very significant and largely unsung pioneer in many advances in anaesthesia.

T. CECIL GRAY

Liverpool

ELECTRICAL AND MECHANICAL ACTIVITY OF THE RAT DUODENUM

Sir,—I am writing concerning the publication "Effects of Inhalation Anaesthetic Agents on the Electrical and Mechanical Activity of the Rat Duodenum," by Wright and colleagues (1982). It is puzzling to me how this publication proceeded through the editorial process of your Journal with one striking error. Presumably, these surgeons were trying to correlate their observations in rats with the occurrence of ileus in postoperative surgical patients. They conclude, "The results presented in this study suggest that of the anaesthetic agents tested, only enflurane is unlikely to contribute to the reduction in intestinal motility which follows operative procedures". I would like to suggest that this conclusion is totally unwarranted for the following reason. Although there was no documentation of the dose of the anaesthetics administered (blood or end-tidal concentrations), in fact the administered concentration of halothane (4%) was twice that of enflurane (2%), when in fact it is rather well demonstrated that enflurane is approximately half as potent as halothane in all species tested thus far (Quasher, Eger and Tinker, 1980). Consequently it seems apparent that what these investigators have observed may well be a dose effect. Before such a conclusion can be drawn, it is incumbent upon these investigators to repeat their experiments using equipotent doses of the anaesthetics.

R. G. MERIN

Houston

REFERENCES


SIR,—Thank you for granting us the opportunity to reply to Professor Merin's comments concerning our paper (Wright et al., 1982). We are puzzled that Professor Merin uses as the basis of his criticism a paper by Quasher, Eger and Tinker (1980) which contains no data on the potency of enflurane in the rat.

We did not measure blood or end-tidal concentrations of the inhalation agents studied because we wished to examine the change in intestinal motor activity at the induction of anaesthesia and, of course, to follow on the recording as anaesthesia deepened. The need for i.v. cannulation or intubation of the rats would have excluded this possibility. We did measure the concentration of anaesthetic drug, in the gas leaving the study chamber, throughout the 30-min study. We considered that this concentration was similar to the inspired concentration. In spite of the different inspired concentrations of the agents used, the

SIR,—I read with particular interest Professor Gray's contribution, having been present at the epoch-making meeting at the Royal Society of Medicine to which he refers (Gray and Halton, 1946). However, an error has crept in—the meeting was on March 1 1946, not 1945.

The account of the discussion following their presentation includes the contributions by Dr Prescott and Dr Organe, as he then was, of work on curare being undertaken simultaneously in London. Regrettably, that by the late Dr E. S. Rowbotham, under whom I was then Resident Anaesthetist at Charing Cross Hospital, was ignored by the reporter. This work was later published (Prescott, Organe and Rowbotham, 1946) and Sir Geoffrey Organe referred to this matter in his paper at the second "Lest We Forget" Symposium at the Royal Society of Medicine (Organe, 1979).

They were indeed "Exciting and dangerous days".

H. A. CONDON

London

REFERENCES


Organe, G. (1979). The introduction of muscle relaxants. Audiocassette of "Lest We Forget-2" meeting; in possession of Association of Anaesthetists.


SIR,—I thank Drs Bready, Swartzman and Adcock, for their comments on my article. I should like to clear up some misunderstandings.

By "possible difficulty" I did not imply likely impossibility. Of course I would not have proceeded had I expected to fail to intubate. When I abandoned attempts to intubate, it was the course of greatest safety at the time. I may have succeeded had I persisted.

I agree that there was no fetal distress in my case, but I considered my actions to be the safest way out of the predicament. The maintenance of the patient's airway required great effort, using one hand, and there was risk of regurgitation. I dealt with the latter by isolating the oesophagus. Spontaneous respiration was then restored. I could then devote both hands to maintaining the patient's airway.

The oropharyngeal airway was only to save effort, not to improve a less than adequate ventilation.

I am not familiar with transcutaneous jet ventilation and would not have proceeded had I expected to fail to intubate. When I abandoned attempts to intubate, it was the course of greatest safety at the time. I may have succeeded had I persisted.

Next time this patient presents for anaesthesia I shall use awake intubation with a fibreoptic bronchoscope, as I consider myself inexpert at blind nasal intubation.

E. BOYS

Bury St Edmunds

WATERTON SYMPOSIUM

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rats were judged at 10 min and 20 min, by the absence of eyelash and the presence of the conjunctival reflex, to be at a clinically similar depth of anaesthesia. Furthermore, respiratory rates were checked at intervals throughout the study and these did not fluctuate once stable anaesthesia was established. The use of this clinical approach was further justified by the similar duration of recovery of consciousness and exploratory activity after enflurane and halothane. The similar clinical depth of anaesthesia at apparently disproportionate inspired concentrations did surprise us but the finding was consistent and, initially, might in part be explained by the different blood-gas solubility coefficients of enflurane and halothane.

If one accepted Professor Merin's interpretation, that the difference between the effect of enflurane and halothane was a result of a dose factor, then it would be reasonable to expect that a progressive change in intestinal activity would occur as anaesthesia deepened. However, this was not the case, the evidence we presented confirmed that the motor response of the small intestine in the rat is not the same for different agents. Therefore, even if, during the course of the anaesthetic period, a deeper level of anaesthesia were achieved with halothane, this did not alter the direction of the results obtained and so we conclude that the difference in the effects of enflurane or halothane on intestinal motility was not dose-dependent. Indeed, a careful study of our paper reveals that the changes in the migratory myoelectric complex (MMC) associated with treatment with enflurane and halothane were not part of a trend. The responses to enflurane included a significant reduction in the duration of the MMC with no change in the duration of phase III activity, whereas halothane was associated with a significant increase in the duration of the MMC and phase III activity was either abolished or of a significantly reduced duration. Furthermore, while the changes after the cessation of anaesthesia using either drug were similar for a 30-min period, that is a return to a pattern approximating to that detected in the normal conscious fasting rat, there were marked abnormalities in the subsequent intestinal motor activity in those rats that had received halothane. We did examine the effect of 5% enflurane and 2% halothane on intestinal motility and the changes recorded were similar to those recorded at the concentrations published (Wright et al., 1982) for both drugs, however the general condition of the rats suggested very deep and very light anaesthesia respectively. These results were not included in our paper because, as a result of the very obvious differences in the levels of anaesthesia, they were not comparable.

We conclude therefore that, while the doses of anaesthetic agents administered for the short period (30 min) of the study were different, comparable levels of anaesthesia were achieved as judged by time of onset of anaesthesia, duration of the awakening periods and the constancy of the conjunctival reflex activity. We do not feel that our concluding sentence was other than, as stated, a suggestion, it was not intended to be taken as a definitive conclusion, but we believe that the suggestion based on our findings was none the less valid.

T. E. J. Healy  
Manchester  
J. W. Wright  
T. W. Balfour  
J. D. Hardcastle  
Nottingham

REFERENCES

VECURONIUM AND ATRACURIUM

Sir—In their recent paper comparing vecuronium and atracurium Robertson and colleagues (1983) raised some interesting points concerning the two drugs.

From their results it can only be claimed that vecuronium has a shorter duration of action than atracurium at the lower dose. At the higher doses (vecuronium 129 µg kg⁻¹ and atracurium 564 µg kg⁻¹) there was no significant difference in duration between the agents. They also noted that the ED₅₀ and ED₉₀ for atracurium (131.1 µg kg⁻¹ and 188.7 µg kg⁻¹, respectively) were lower than those found by other authors and, thus, the role of anaesthetic agents in affecting the results needs to be considered. Folds and colleagues (1982) found the ED₅₀ and ED₉₀ for vecuronium to be 21.5 and 26.7 µg kg⁻¹, and for atracurium the values were 103.0 and 167.2 µg kg⁻¹, respectively.

It has been shown by Thompson, Merret and Webb (1982) that ester hydrolysis does not play a significant part in the elimination of atracurium. The Hoffman Elimination described in 1851 by A. W. Hoffman involved the decomposition of quaternary ammonium salts when heated with strong alkalis. With atracurium, electron withdrawal because of the positive charges on the nitrogen is thought to weaken the B - C - H bond, thereby assisting proton transfer (Stenlake, 1982). This reaction occurs at a physiological temperature and pH and requires no enzymes. Thus, there is no metabolic pathway as such to be overloaded and it is not surprising that the elimination has been observed to be a constant reaction (Hughes and Chapple, 1981; Payne and Hughes, 1981) since biological variables, such as enzyme concentrations and hepato - renal function play no part in the drugs removal (Utting, Hunter and Jones, 1982).

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Peterborough

REFERENCES